

What the “simple renormalization group” approach to dark matter clustering really was

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McDonald (2007) presented an approach to improving perturbation theory (PT) calculations of the dark matter power spectrum, with a derivation based on the idea of renormalization group flow with time. In spite of a questionable approximation made in deriving it, subsequent comparisons by several groups between the predictions of the resulting equation and N-body simulations showed remarkable improvement relative to “standard” PT (SPT) at similar order. In this brief note I show that the same final equation can be derived cleanly from the point of view not of flowing with time but with non-linear coupling strength, i.e., gradually dialing the coupling from the trivial value zero to the physical one. This understanding makes it clear how to extend the approach to higher order and other statistics. While I do not necessarily think this approach is best among the many, it may be interesting in that it contains a unique way of suppressing UV sensitivity. In passing I remind the reader of references demonstrating that SPT works remarkably well without improvement (except near redshift zero, where, fortunately for SPT, there is very little volume in the Universe).

I. INTRODUCTION

Recently there has been a lot of work toward improving perturbative predictions for dark matter clustering [1–24]. One could argue that this is an academic indulgence, because we can use N-body simulations to calculate the same results essentially exactly from first principles; however, we will never be able to simulate galaxies and other tracers of dark matter anywhere near exactly from first principles, so in my opinion it is vital to develop a deeper understanding of what forms of large-scale clustering are generally possible [25–45]. The power of future high precision large-scale structure surveys to constrain fundamental physics will depend sensitively on how well we can model these tracers [46, 47]. Understanding dark matter clustering beyond what is possible by running simulations is a prerequisite for more general understanding of large-scale structure.

In [1] I presented one relatively early approach to improving “standard” (in the sense of being what people have done for a long time [48–53]) Eulerian perturbation theory (SPT). The calculation in [1] was motivated by the general renormalization group approach of [54], and explained as an implementation of that method; however, a complete implementation of the method of [54] would have required non-negligible work beyond standard perturbation theory. Finding that a corner-cutting version of the calculation seemed to work well by comparison to simulation fitting formulas, [1] swept the conceptual deficiencies under the rug and declared victory. Specifically: the method of [54] is essentially to reset the initial conditions (IC) for perturbation theory at a continuous series of time steps, absorbing the corrections in the previous step into the initial conditions for the next step. Implementing the method properly would have taken more work because the corrections don’t take the form of growing modes, but standard PT is written for growing mode initial conditions. To be clearer: at 2nd order in PT δ and θ are not simply equal to each other as they would be for linear theory growing modes (when appropriately defined). This means that the 2nd order results cannot be simply plugged back in place of the usual linear theory initial conditions – to do this properly requires deriving the PT evolution for a short time after an arbitrary start (normally the use of only the fastest growing modes is justified by the fact that the starting point is effectively infinitely far in the past). This is not actually very hard, but [1] took the easy way out after the embarrassingly poorly motivated “approximation” of ignoring the sub-leading modes appeared to succeed (if it is not obvious why we shouldn’t expect this to be a reasonable approximation, it is because we know that a non-negligible part of the SPT prediction flows for some time through these modes – it is really only in the infinite-past initial conditions that they are negligible).

[1] was aware of the deficiencies of the calculation and suggested that one way forward might be to simply write down and numerically evolve the full evolution equations for statistics, i.e., the power spectrum, bispectrum, trispectrum, etc., an idea that was implemented as the “time-RG” method by [55]. However, predictions of the equations of [1] were subsequently compared in more detail to results from N-body simulations by several groups, finding remarkable

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improvement over standard perturbation theory, for a diverse set of scenarios. [56] simulated a wide range of power law initial conditions, from $n = -1$ to $n = -2.5$, finding (their Fig. 5) that the RG predictions were always a substantial improvement over the 1-loop SPT result. [57] did not include the method of [1], which they call RGPT, in most of their comparisons (possibly because I was telling them at the time that they should consider “time-RG” to be the natural evolution); however, where they do show it (the cyan line in their Fig. 3) we see excellent agreement at $z = 1$. Basically, we see improvements comparable or somewhat better than going from 1-loop to 2-loop SPT (which, as we see in Fig. 1 of [57] is impressive in itself at $z = 1$ – I will discuss this, including redshift dependence, more below), so we can either be happy not to need to deal with the pain of the 2-loop calculation, or we can hope for better by taking the RG method to higher order. Finally, [58] present simulations of an interesting combination of different power laws (from $n = -0.5$ to -1.5) with a BAO-like feature, showing (their Fig. 11) that “coupling strength” RGPT (named based on a draft of this note) performs well. To be fair, the results in these papers are far from perfect, but they show that the method generally does what it is supposed to, improve SPT, and does it arguably as well as any others [57]. It may be that other methods derived more recently are better, but maybe we just needed to work harder on this one. This motivates this note in which I show how to derive the results of [1] without hacking, and then how to extend it to higher order and other statistics.

II. RE-DERIVATION AND INTERPRETATION OF THE “SIMPLE RG” RESULT

The basic form of the SPT solution, defining $A = D^2$, is:

$$P(k, A, \lambda) = AP_L(k) + \lambda A^2[P_L^2](k) + \lambda^2 A^3[P_L^3](k) + \dots \quad (1)$$

where $[P_L^n]$ are shorthand for the convolution of n power spectra that one finds in the n th order SPT calculation, and λ is a non-linear coupling strength parameter. I have written $P(k, A, \lambda)$ to emphasize that I have generalized the solution to apply for any coupling strength, including $\lambda = 0$, where linear theory works perfectly by definition, and the true physical value $\lambda = 1$. The strategy here will be to use an RG approach to “turn on” the non-linear coupling, in some sense evolving the solution from the known starting point $\lambda = 0$ to the point of interest $\lambda = 1$. Because of the simple form taken by time evolution in LSS PT, this approach looks very much like a time evolution, but that is really not what we are doing here.

The key to renormalization is to exploit a general ambiguity inherent in perturbation theory, that the perturbative “order” of different quantities in the calculation is not god-given – it is up to the user to define things in a way that gives the best possible behavior of the resulting series. The “standard” LSS perturbation theory calculation assumes that the initial conditions $P_L(k)$ go entirely into the lowest order solution, but this is not mathematically necessary. If we allow for IC at each order, $P_1(k)$, $P_2(k)$, and $P_3(k)$, we find

$$P(k, A, \lambda) = A(P_1(k) + P_2(k) + P_3(k)) + \lambda A^2([P_1^2](k) + 2[P_1P_2](k)) + \lambda^2 A^3[P_1^3] + \dots \quad (2)$$

where note that the exact same solutions apply regardless of the order label on the initial conditions – the label just determines which terms to drop (to be clear, $[P_1P_2]$ is the usual 1-loop convolution using P_1 for one of the power spectra and P_2 for the other, symmetrically of course).

For any given value of λ , call it λ_* , we can specify P_2 and P_3 to make the beyond-linear corrections zero, i.e.,

$$P_2(k, \lambda_*) = -\lambda_* [P_1^2(\lambda_*)] \quad (3)$$

where the leading order result has become a function of λ_* , $P_1(k, \lambda_*)$, and

$$P_3(k, \lambda_*) = -2\lambda_* [P_1(\lambda_*)P_2(\lambda_*)] - \lambda_*^2 [P_1^3(\lambda_*)] . \quad (4)$$

We suppress the A dependence of everything in these equations as it plays no role (one should think of the calculation as being done at some fixed A). The complete solution as a function of λ is then

$$P(k, \lambda) = P_1(k, \lambda_*) + (\lambda - \lambda_*) ([P_1^2(\lambda_*)](k) + 2[P_1(\lambda_*)P_2(\lambda_*)](k)) + (\lambda^2 - \lambda_*^2) [P_1^3(\lambda_*)] + \dots \quad (5)$$

Now, as usual in RG calculations, we enforce the fact that the solution should not depend on the arbitrary cancellation point λ_* , i.e., the derivative of the solution, $P(k, \lambda)$, as laid out in equation 5, with respect to λ_* should be zero. This should be true at any value of λ_* but for convenience we take this derivative at $\lambda_* = \lambda$, i.e., set $\left. \frac{dP(k, \lambda)}{d\lambda_*} \right|_{\lambda_* = \lambda} = 0$, to derive:

$$\frac{dP(k, \lambda)}{d\lambda} = [P^2(\lambda)](k) + 2\lambda ([P^3(\lambda)] - [P(\lambda)[P^2(\lambda)]]) + \dots \quad (6)$$

where I have used the fact that $P(k, \lambda) = P_1(k, \lambda)$ and $P_2(k, \lambda) = -\lambda [P_1^2(\lambda)]$, when $\lambda_* = \lambda$.

The first term here is the result of [1]. The 2nd shows how to go to the next order. To be clear, $[P^2(\lambda)](k)$ is the usual 1-loop SPT calculation, with $P(k, \lambda)$ as the input power spectrum, $[P^3(\lambda)](k)$ is the usual 2-loop SPT calculation, and $[P(\lambda) [P^2(\lambda)]](k)$ means do the 1-loop SPT convolution with one of the two input power spectra (symmetrically) replaced by the 1-loop result itself. The initial condition for solving the differential equation is $P(k, \lambda = 0) = P_L(k)$, i.e., at zero non-linear coupling the solution is obviously the linear solution.

III. EXTENSION TO OTHER QUANTITIES

It should be clear at this point how to extend this calculation to other statistics. For example, redshift space distortions in the distribution function approach [59–63] require predictions for statistics involving momentum, motivating computing $\dot{\delta} \equiv d\delta/dt$ which is equivalent to the divergence of momentum through the continuity equation. I also show how to compute the propagator of [64, 65].

A. $P_{\delta\delta}$

We can compute $P_{\delta\delta} = \frac{1}{2} dP_{\delta\delta}/dt$ by evaluating Eq. 6 at two nearby times to take a numerical derivative, or directly through:

$$\frac{dP(k, A, \lambda)}{d \ln A} \equiv P'(k, A, \lambda) = A (P_1(k) + P_2(k) + P_3(k)) + 2\lambda A^2 ([P_1^2](k) + 2[P_1 P_2](k)) + 3\lambda^2 A^3 [P_1^3] + \dots \quad (7)$$

and then, following the same calculation as above,

$$P_2(k, \lambda_*) = -2\lambda_* [P_1^2(\lambda_*)] \quad (8)$$

$$P_3(k, \lambda_*) = -4\lambda_* [P_1(\lambda_*) P_2(\lambda_*)] - 3\lambda_*^2 [P_1^3(\lambda_*)] \quad (9)$$

$$P'(k, \lambda) = P_1(k, \lambda_*) + 2(\lambda - \lambda_*) ([P_1^2(\lambda_*)](k) + 2[P_1(\lambda_*) P_2(\lambda_*)](k)) + 3(\lambda^2 - \lambda_*^2) [P_1^3(\lambda_*)] + \dots \quad (10)$$

$$\frac{dP'(k, \lambda)}{d\lambda} = 2 [P'^2(\lambda)](k) + 2\lambda (3 [P'^3(\lambda)] - 4 [P'(\lambda) [P'^2(\lambda)]](k)) + \dots \quad (11)$$

using $P'(k, \lambda) = P_1(k, \lambda)$. While the form is similar to equation 6, there is really no simple analytic relation between $P'(k, A, \lambda)$ and $P(k, A, \lambda)$.

B. $P_{\delta\dot{\delta}}$

If we are interested in computing $P_{\delta\dot{\delta}}$, we must go back a step, as this cannot be written as a simple derivative of the power spectrum. The PT density field is

$$\delta(k, D, \epsilon) = D\delta_L(k) + D^2\epsilon[\delta_L^2](k) + D^3\epsilon^2[\delta_L^3](k) + \dots \quad (12)$$

$$\frac{d\delta(k, D, \epsilon)}{d \ln D} = D\delta_L(k) + 2D^2\epsilon[\delta_L^2](k) + 3D^3\epsilon^2[\delta_L^3](k) + \dots \quad (13)$$

or (tracking only 1 loop here for simplicity)

$$P_{\delta'\delta'} = AP_L(k) + 4A^2\lambda [P_L^2]_{22}(k) + 6A^2\lambda [P_L^2]_{13}(k) + \dots \quad (14)$$

where we have defined $[P^2]_{22}$ and $[P^2]_{13}$ such that the standard PT density power spectrum result would be $P_L + [P_L^2]_{22} + 2[P_L^2]_{13}$ (P_{13} may sometimes be defined to include the factor of 2). Defining $[P^2]_{\delta'\delta'} = 4[P^2]_{22}(k) + 6[P^2]_{13}(k)$ we have

$$P_{\delta'\delta'} = A(P_1(k) + P_2(k)) + \lambda A^2 [P_1^2]_{\delta'\delta'} \quad (15)$$

i.e.,

$$P_2(k, \lambda_\star) = -\lambda_\star [P_1^2(\lambda_\star)]_{\delta'\delta'} \quad (16)$$

i.e.,

$$\frac{dP_{\delta'\delta'}(k, \lambda)}{d\lambda} = [P_{\delta'\delta'}^2(\lambda)]_{\delta'\delta'}(k) . \quad (17)$$

The equation to be solved again has similar form but non-trivially different coefficients.

C. Propagator

Another thing we can compute is the propagator, $P_{\delta\delta_L}/P_L$, i.e., the standard PT result is

$$P_{\delta\delta_L}(k, A, \lambda) = AP_L(k) + A^2\lambda [P_L^2]_{13} \quad (18)$$

and we see immediately that the RG equation is

$$\frac{dP_{\delta\delta_L}(k, \lambda)}{d\lambda} = [P_{\delta\delta_L}^2(\lambda)]_{13}(k) \quad (19)$$

where again note that $[P^2]_{13}$ is defined to account for only a single product $\langle\delta_1\delta_3\rangle$.

IV. DISCUSSION

It should be clear from these examples how to derive any other desired quantity. E.g., $P_{\theta\theta}$ would follow equation (6) with the relevant kernels for θ replacing those for δ . I am probably setting a bad example with these extensions, however, as it is probably best to use this RG method only to compute complete observables of interest, not to compute sub-components of observables separately with the intention of adding them, because that hides potentially exact cancellations from the method. This is exemplified by comparing the result for the propagator, equation (19) to the full power spectrum, equation (6). We can see looking at equation (19) that it will produce the expected behavior of a propagator, as discussed in detail by [64, 65]. Because of the form of $[P^2]_{13}(k)$, proportional to $P(k)$, we could re-write equation (19) as an equation for $\frac{d \ln P_{\delta\delta_L}(k, \lambda)}{d\lambda}$ so that, considering the general negativity of $[P^2]_{13}(k)$, it is clear that $P_{\delta\delta_L}$ will stay positive but be driven to zero at high k , as we expect for the propagator. [64, 65] presented this propagator as a fundamental building block of the calculation of the full power spectrum, but this turns out to be problematic in principle (although maybe not always in practice). The problem is that the propagator is sensitive to large-scale bulk flows, with one well-known symptom being that $[P^2]_{13}$ is infrared divergent for sufficiently red power spectra, when the velocity variance is also divergent. This is a physically correct result as far as the propagator goes – a large homogeneous velocity field really will decouple the initial conditions from the final field in fixed Eulerian coordinates, as defined by $P_{\delta\delta_L}(k)$, and the RG equation (19) will do the right thing by sending the propagator to zero instead of negative infinity (for any reasonable regulation of the divergence). However, it is easy to see that this potential infrared divergence is not relevant for the autocorrelation of the evolved field, or any correlation of fields that are moving together with the bulk flow, because of Galilean invariance. Mathematically, this irrelevance of the potential infrared divergence in P_{13} is correctly manifested in the SPT calculation of the autocorrelation because the P_{13} divergence exactly cancels against a similar divergence in P_{22} . The RG equation (6) for the autocorrelation “understands” this exact cancellation and therefore behaves fundamentally differently from the combination of separate P_{13} and P_{22} calculations. While P_{13} and P_{22} do not actually diverge for the Λ CDM power spectrum, the same physical intuition should apply more generally: it is clearly desirable to apply renormalization to fully observable quantities, where symmetry-enforced exact cancellations are manifest.

As discussed in [1], the RG equation (6) has another interesting property related to the fact that it combines 13 and 22 terms – it has a fixed-point solution (at 1-loop order) at a $k^{-1.4}$ power spectrum, i.e., the power spectrum where 1-loop SPT corrections have been shown to be exactly zero [66]. This is an attractive fixed point, i.e., a general power spectrum evolving under equation (6) converges toward it, most quickly on small scales, as shown in Figure 1. This is interesting in that it erases sensitivity to small-scale details in a really unique way – producing universal non-trivial small-scale behavior that is grafted onto the model-dependent large-scale perturbative behavior. This differs qualitatively from something like the propagator renormalization approach, which completely erases small-scale structure at lowest order and then needs to build it back up through loops [64, 65], or the effective field theory

(EFT) approach [4] which removes small-scale sensitivity at the expense of introducing a free parameter representing an effective sound-speed or viscosity for the effective fluid. While I generally like the EFT approach, this raises the question of whether or not that kind of free parameter is really necessary. It is easy to see situations in which it unquestionably would be: if the system under discussion contained fundamentally small-scale, highly non-trivial physics, e.g., star formation and supernova explosions setting the temperature of some gas, obviously one is going to need to represent that temperature by a free parameter. This is pretty close to exactly the situation with tracers of dark matter like galaxies, so an EFT approach with free parameters is pretty obviously the best way to think about biasing [25, 67]. On the other hand, is it possible that in certain simpler situations like the clustering of dark matter, where there are important conservation laws at work, that the EFT parameters are more a property of the “boundary layer” non-linearities, accessible to perturbative calculations near that boundary, instead of fundamentally sensitive to the initial conditions and things well below the non-linear scale? I have in mind a picture in which the formation of halos is like the closing of a box around the interior of the halo, with the large-scale clustering subsequently only dependent on simple properties of these boxes, not what is happening inside them, and with those properties determined by the structure on the presently collapsing scale, not what happened earlier inside. A simple example is to imagine a pair of sub-objects tightly orbiting each other inside the halo – clearly you can make the binary tighter and change the kinetic energy in the halo, but this isn’t going to have any effect on the large-scale clustering of the halo, and therefore you don’t need to be able to predict it (on the other hand, the details of this binary definitely would affect the redshift-space power spectrum, so this thought experiment implies the need for a free parameter there, even for something as simple as dark matter). This picture of universal halos clustering independent of their internal details obviously isn’t anything new [68–70], the question is just, does it imply that EFT parameters could be predictable from first principles? The attractive fixed-point behavior seen in Figure 1 suggests a way that this kind of predictable self-arrangement of the EFT could work...

To randomly change the subject, I’d like to comment on the relation of the power spectrum and its prediction to the correlation function. The most proximate motivation for this is the horrible jagged line shown in Fig. 14 of [58] as the transformation of the RGPT power spectrum prediction to a correlation function (accompanied by the statement that they couldn’t reasonably transform the SPT power spectrum at all), but I think there is more generally a lot of confusion about this issue. The first thing one usually thinks is “ $P(k)$ and $\xi(r)$ are just linear transforms of each other, so they really should be equivalent” which is then quickly followed by “but this is only true if one considers all values of k and r , which is generally impractical, so really they can be different.” I think one should look very skeptically at any conclusion that they are different in an important way. The first thing to note is that the naive correlation function $\langle\delta(\mathbf{x})\delta(\mathbf{y})\rangle$ is generally a math/physically pathological statistic, because these δ ’s are defined at mathematical points, and the statistic is therefore generally sensitive to arbitrarily small-scale physics. This is easy to see by considering the correlation function of a regular lattice of point-like objects, which will have delta function spikes at multiples of the lattice spacing. Clearly the form of these spikes is sensitive to the detailed small-scale structure of the objects, no matter how large the separation in the correlation function. If we do not want the correlation function to be sensitive to small scale structure in this way, we need to apply some smoothing, enough to smear out the details of the point objects. This smoothing is obviously equivalent to restricting ourselves to considering the low- k part of the power spectrum. This general small-scale sensitivity of the un-smoothed correlation function is obscured in cosmology because the small-scale structure is not arranged on a regular grid, so it is naturally smeared, i.e., there is no significant small-scale structure in the correlation function at large separations. The BAO feature is a notable exception to this, where the discussion above explains why it is affected by non-linearities that one would not notice at similar separation if the feature was not present. The relevance to perturbation theory predictions is: it is a mistake to ask any method to predict the un-smoothed correlation function. The correct procedure is to ask: “what level of smoothing can I apply to the correlation function without erasing cosmological structure that I believe to be present and predictably relevant to my cosmological parameter estimation,” and then consider predictions given this smoothing. This is only one issue to be careful about, associated with the relation between high- k and small *difference* in r effects. There are other potential stumbling blocks associated with other limits of the coordinates. I think the correct way to look at this issue is to understand that sometimes $P(k)$ can be more convenient to work with than $\xi(r)$, or vice versa, and when they appear to imply fundamentally different conclusions it is probably because you are trying to ignore the inconvenience of one of them, rather than dealing with it appropriately, not because they really are fundamentally different.

Finally, since I have been hearing a lot lately in talks and conversation the unqualified statement that “SPT doesn’t work”, which seems to blatantly contradict previous conclusions, it is useful to review the literature on this, which turns out to be very interesting. The short summary is that it would be more accurate to say that “even though SPT works very well where we need it to, one can imagine a different Universe in which it wouldn’t, so it is fun to think about that”, or, more practically “SPT works great up to 2 loops but can’t be pushed beyond that to give very high accuracy, so some improvement is desirable”. First, the old positive result one remembers is certainly correct, e.g., [71] show that 1-loop SPT gives an obvious large improvement over linear theory at all redshifts they compute, $z = 1$

and higher (see also [72]). [57] confirms this result and adds a 2-loop calculation, which again dramatically improves the results over 1-loop at $z = 1$ (see also [73]). Here we see, however, that while the 1-loop calculation offers some improvement over linear theory at $z = 0$, where the amplitude of the linear power is a lot higher, the 2-loop calculation arguably actually makes things worse. This is intriguing, but not very relevant as there is very little volume in the Universe near $z = 0$. Things get really exciting in the 3-loop calculation of [74] (if you are going to look at one figure in another paper related to this discussion, it should be the $z = 0.833$ panel of their Fig. 2), who show that, after appearing to be converging *beautifully* at 1 and 2-loops at $z \sim 1$, the 3-loop SPT result is suddenly catastrophically wrong (this was anticipated by [75]). [74] explains that this is not so much a divergence of the wavenumber integrals involved as an explosion of the combinatoric factor in front of the terms. Apparently this is well known behavior in quantum field theory, including even QED [76], where the series expansions are generally also asymptotic, not convergent. This is not considered to be a big fundamental problem, e.g., in identifying the issue in QED, [76] was careful to state in the abstract that “The divergence in no way restricts the accuracy of practical calculations that can be made with the theory” – the solution is simply to not calculate beyond the point where the terms in the series are getting smaller. While this is in practice a hundred or more terms in QED, because the coupling constant is so small, it is clearly more of a practical problem for SPT, because, while the 2-loop result is great at $z \sim 1$, it will have larger errors than the statistical errors of a survey like DESI [46] once one pushes to high enough k , and clearly simply going to 3-loops is not an option (although, on the other hand, one has to wonder if this will be the limitation once the unavoidable bias parameters are marginalized over). Note that [74] show that the 2-loop calculation is pretty good for z even as low as 0.375, i.e., the really bad breakdown only happens at a completely irrelevantly low redshift (there is > 20 times as much volume in the Universe in the range $0.4 < z < 1.5$ as $z < 0.4$, and of course a lot more at higher z). To be clear, I don’t think this invalidates the goal of improving on SPT, I just think the motivation should be presented more accurately – the motivation is abstract mathematical physics interest, i.e., applicable to Universes other than ours, and/or it is detailed improvements on the success of SPT in our Universe.

To conclude, the attractive fixed-point behavior dominating small scales in equation (6) is a reason to be intrigued by this method, but I found that it also contains a practical drawback: you do need to do the wavevector integral out to high q to preserve this behavior, and therefore need to track the evolution of the power spectrum out to high q , and straightforward numerical evolution of the differential equation tends to grind to a halt with impossibly small steps. This problem is not insurmountable, and several other groups have also overcome it, but it is something to be aware of – you don’t want to set out to evolve equation (6) with the assumption that it will be trivial.

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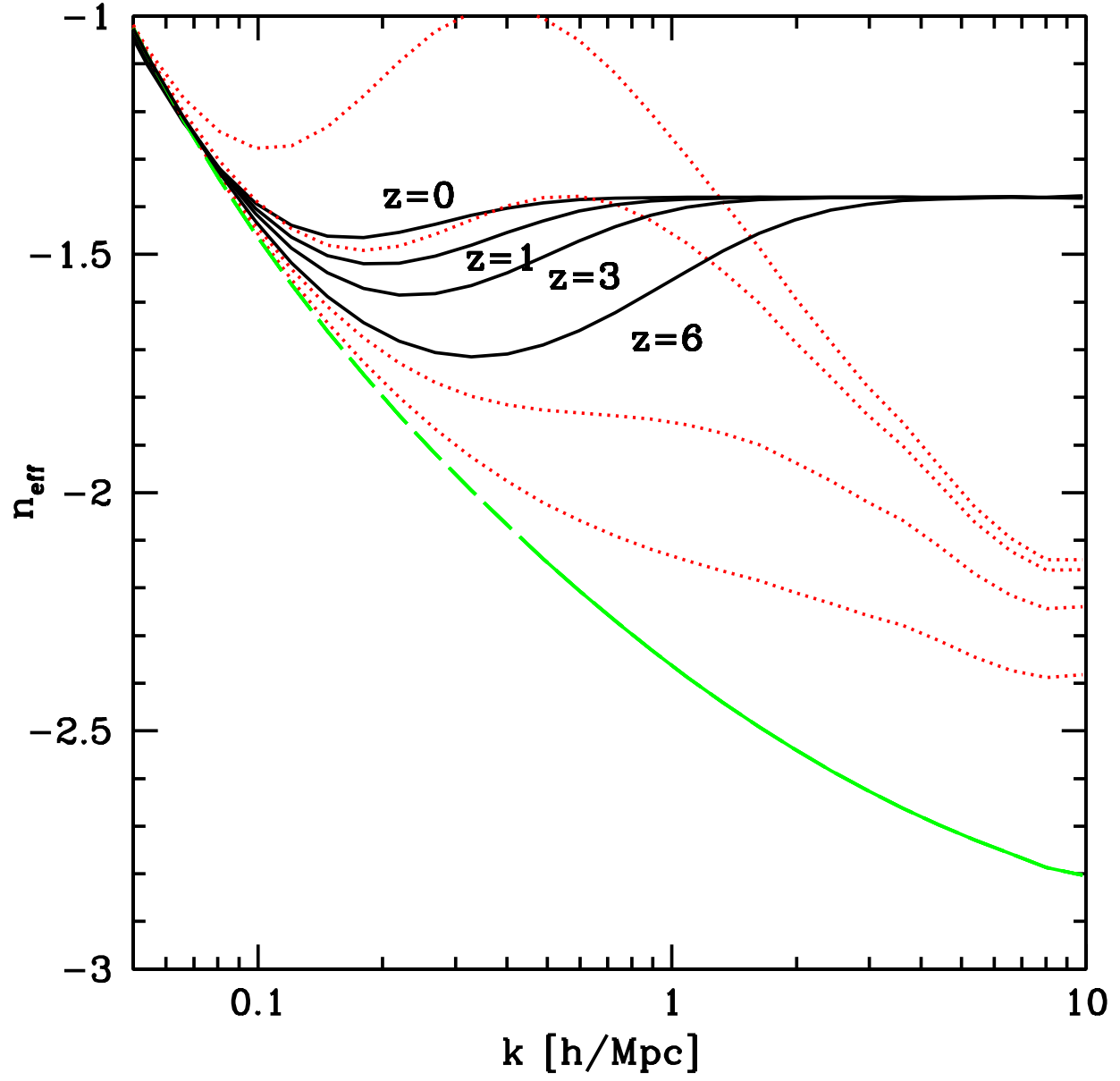


FIG. 1. The green (dashed) line shows $n_{\text{eff}}(k) \equiv \frac{d \ln P}{d \ln k}(k)$ for the linear power spectrum. Black (solid) lines show $n_{\text{eff}}(k)$ for the RG power spectrum (equation 6, at lowest order) at redshifts from top to bottom $z = 0, 1, 3, 6$. Red (dotted) lines show $n_{\text{eff}}(k)$ for the 1-loop SPT power spectrum at the same redshifts.